ANALYSIS

Edited by A. E. Duncan-Jones with the cooperation of L. S. Stebbing C. A. Mace and G. Ryle

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THE TRUTH OF EMPIRICAL STATEMENTS

By B. von Juhos

LANGUAGES can be constructed, which are completely describable in a purely formal way : i.e. within the description we meet only with notions, which refer to the expressions and the signs of the language and the fixed rules of their order. Languages which can be described in this purely structural way are called " formalised ".[1]

Usually, that is also in the language of empirical science, truth is explained as " conformity of a statement with reality ". Recent investigations have proved that in ordinary language the use of the concept of truth is not free from contradiction.[2] But all the same it is conceivable that for empirical statements of a correspondingly formalised language a definition of truth can be found which, besides being free from contradiction, will satisfy the following conditions. Firstly, the definition is made with the help of purely logical concepts, or of concepts which belong either to the formalised language itself or to the structural description of this language, and secondly it shall suggest as plausible the interpretation according to which truth is to be designated as " conformity with reality ". *A. Tarski* has made this attempt, and believes that he is able to give for languages which are structurally

[1] Cf. the following exposition with *A. Tarski*, " Grundlegung der wissenschaftlichen Semantik " in " Actes du Congrés international de philosophie scientifique", Paris 1935 ; Herman & Cie, Paris 1936, pp. 1–8. All quoted passages are translated by me.

[2] Cf. *R. Carnap*, " Die Antinomien und die Unvollständigkeit der Mathematik " in " Monatshefte f. Math. u. Physik," Vol. 41, No. 2, p. 266 ff.

describable in a definite way a definition of truth (in the meta-language constructed as an addition to the language in question), which satisfies the two postulations which have been mentioned. But in my opinion his definition of truth, whose importance for the treating of analytical problems we certainly do not doubt, does not satisfy both the conditions stated : and on grounds of principle it is impossible to give such a definition at all.

If we speak here about empirical statements, we shall always mean by this empirical statements of a formalised language. If we try to define the truth of such statements, we have to make the following choice : either we establish a definition satisfying the formal postulations mentioned above, but then we renounce the possibility of interpreting this definition in a sense related to the usual mode of speech, or we give a definition, which allows and suggests an interpretation of truth as " conformity with reality ", but as a result of which certain essential formal postulations will have to be given up. We will clear up these conditions in relation to Tarski's definition (Cf. l. c., p. 4 ff.).

Tarski's method is to give a system of statements, which have the form of an equivalence, and the character of partial definitions. These statements then define " the meaning of the concept under consideration—in our case the concept of truth—with regard to all concrete, structurally described expressions " of the formalised language in question. Such partial definitions run for instance as follows : " the statement ' it snows ' is true if and only if it snows ", and so on. They all have the form : " the statement *x* is true if and only if *p* " ; Tarski adds to this form the following explanation : " *p* " shall " be replaced by any statement of the examined language and ' *x* ' by any specific-name of this statement, subject to the condition that this specific-name belongs to the sphere of the meta-language " (1 c. p. 4).

Tarski's definition of truth thus has the form of an equivalence between statements, or more precisely, between expressions of different type, which can be completed into statements by appropriate substitution. Equivalence is a logical relation : how is it used? The statement " *r* is equivalent to *s* " can be expressed in other words as follows : " *r* and *s* are always both true, or both false ". The equivalence states something about the truth-value of the antecedent as well as the consequent. If we apply this strict meaning of equivalence to Tarski's definition

of truth, which is supposed to have the form of an equivalence, we meet with obscurities. And as a definition cannot avoid the form of an equivalence, this obscurity in my opinion arises from grounds of principle.

Let us interpret the definition " the statement *x* is true if and only if *p* " in terms of the explanation added by Tarski. Accordingly " *p* " is to be replaced by any statement of the examined language, " *x* " by a specific-name of this statement, belonging to the meta-language. " *x* " and " *p* " are therefore of different types. But is it consequently possible to say about these expressions : " *x* " and " *p* " are both true or both false ? Hardly without a special, differentiating interpretation of the concept of truth. But Tarski's definition is intended to be an equivalence, and must therefore also be formulable as follows :

(I) " the statement *x* is true if and only if *p* is true."

We by no means wish to describe this formulation of Tarski's definition, which results in our opinion from the explanation given by him, as vicious. The argument-values of " *p* " and " *x* " belong to different languages, i.e., to the examined language and the corresponding meta-language respectively. And someone might adopt the point of view that in correspondence with the hierarchy of languages and the different types of argument-values, " truth " ought also to be graduated in several types. Then " the truth of *x* " would be defined by the logically distinct " truth of *p*," which would not be circular at all. For merely formal systems, where only proofs of analytical consistency or of analytical determinability are of interest, definition (I) may be very effective. But it ceases to be available where " the truth of empirical statements " is concerned. Above all, in this form it does not contain any pretext for the interpretation of truth as " conformity of a statement with reality ". For the " truth of *p* " in this form is introduced *undefined*. It is thus presupposed that we already knew what is meant by the " truth " of a statement of the language under consideration, and thus exactly what it is that we should call " the truth of an empirical statement " ; and the definition does not contain any indication that this undefined truth is to be understood as " conformity with reality ". Moreover we neither want to raise the question whether nor to deny that, in spite of the difference of the types of " *x* " and " *p*," we speak in (I) of only *one* kind of truth.

It may be that even if this is so (I) is useful for purely analytical investigations, but about the truth of empirical statements nothing would be settled by this interpretation. Interpreted thus (I) would fit *all* cases (as the definiendum appears also in the definiens), whether we want to mean by " truth " conformity with reality, or arbitrary establishment or the result of a kind of voting or anything else. But that is why this definition is not appropriate to what we usually call the " truth of empirical statements."

Let us now try another interpretation of Tarski's definition of truth, which takes into consideration primarily the ordinary way of speaking, according to which truth means " conformity with reality ". The definition " the statement x is true if and only if p " can also be completed as follows :

(II) " the statement x is true if and only if p is the case (i.e. if what p states is the case)."

In this form, if we replace x by a specific name of p, which we will call T, the definition suggests the interpretation : " the statement T is true, if it is in conformity with reality ". But now the following point must not escape our notice. The right hand side of the " equivalence " now does not say anything about the truth-value of p, and expresses something not representable in the formal mode of speech. The consequence is that the definition now only has the form of a logical equivalence in outward appearance. In fact the relation expressed in the definition is now *not allowed* to be represented by the logical constant " $\equiv$". For the definition now does not express that two statements (or complexes of statements) are always both true or both untrue, but that a statement is *true, if a certain fact is actually the case.* And this relation is not an equivalence and not a logical-formal relation at all, or a logical concept. Such an " equivalence " is not structurally describable, and therefore cannot belong to a formalised language as a concept, nor does it itself mean a structural description. In the form (II), it is true, the definition of truth does reproduce what we mean by the " truth of empirical statements " in the usual sense, but it does not satisfy the logical requirements laid down by Tarski, in a more general sense.

Someone might reply that even in form (II) the representation of the definition of truth as a formal equivalence might be obtained, if we take the following possibility into consideration.

The definition " the statement *x* is true if and only if *p* is the case" shall be taken in the sense that *x* is true if and only if the statement " *p* is the case " is true. In this way the definition would certainly regain the form of a formal equivalence, but also at the same time form (I), and (setting aside the new difficulty of describing structurally the concept " x is the case ") we should again meet with the objections raised against (I). The objections mentioned here against the attempts at defining the truth of empirical statements, as far as these are represented only in formalised languages, seem to me to rest on grounds of principle. So far as we want to give a definition containing only logical concepts or concepts which belong to the formalised language or to the structural description of it, such a definition leaves absolutely uncertain what meaning we are to attach to the truth of an empirical statement, and in particular does not render plausible an interpretation by means of which truth as defined might be interpreted as " conformity of the statement with reality ". (Objections to (I).) As soon as the latter interpretation is demanded, it can only be obtained by violating the postulations mentioned above. (Objection to (II).) The reason for these difficulties seems to me to lie in the fact that the equivalence-relation has the property of stating something about the " truth " of the antecedent as well as of the consequent. That is the reason why in my opinion it is hardly possible to define the truth of empirical statements with the help of the formal equivalence-relation. That Tarski's truth-definition in the form given by him, " the statement *x* is true if and only if *p*," seems to satisfy the logical postulations, as well as suggesting an interpretation in the sense of empirical truth, is to be traced back in my view to the inadmissible brevity, i.e. incompleteness, of this definition, which does not give us the means of deciding whether by " equivalence " is meant a logical-formal, or a non-logical and also structurally non-describable relation.

From this point of view we may also define our attitude to Tarski's opinion that about the "truth" of empirical statements also (cf. l. c., p. 2 ff.) one can only speak in relation to a certain language. This view is certainly well substantiated to the extent that it is always only *meaningful* statements which can be true or false. The meaning of a statement is fixed by linguistic rules, which indicate how the statement in question is to be composed

of signs, how it is to be used and treated.[3] If we change the rules according to which an empirical statement is used, that is, if we alter the meaning of *p*, then of course its truth-value may also change. Enough rules of the language in question must therefore already be fixed before we can ask significant questions as to the truth-value of a statement expressed in the language in question. Thus we agree with Tarski that the truth of a statement depends up to a certain point upon certain linguistic rules. But if Tarski believes further that it is possible in relation to certain " formalised " languages to define the truth of empirical statements by the help of logical concepts, or such concepts as belong to the language in question itself or to its structural description, and thus to give a definition of truth in such a case by means of purely formal linguistic rules and specifications in a broader sense, and that such a definition may plausibly be interpreted as " conformity of the statement with reality ", we must contradict him. The reasons we have given in this paper. In our view it is impossible to define the truth of empirical statements even in formalised languages in the way demanded by Tarski, that is to say the truth of empirical statements is an *extra-logical, extra-linguistic* concept (i.e. a concept falling outside the language under consideration and the meta-language belonging to it.)

The fact must again be stressed that our arguments are not intended to bring into question the great importance of Tarski's truth-definition for analytical and formal problems, as long as it is not supposed to be interpreted as " truth of empirical statements.'

Vienna

[3] A clear and easily understood exposition of the dependence of the meaning of empirical statements and expressions upon linguistic rules is given by *J. Schächter*, " Prolegomena zu einer kritischen Grammatik," Wien 1935.

REDUCTION-SENTENCES AND EXTENSIONALITY

By WILLIAM BARRETT

1. The thesis of physicalism had been stated by Carnap (*Logical Syntax*, p. 151) in such a sentence, among others, as : " " A is furious " or " A is thinking " means : " The body A (i.e. such and such a space-time domain) is in such and such a state " ". But a formulation of this kind is hardly satisfactory, not only because of the ambiguous word " means " here, but because too the sentence conveys the sense of " nothing but "—that being furious or thinking are " nothing but " space-time processes—in short, a traditional Metaphysical reduction with which certain materialistic doctrines have made us familiar. Precisely in order to correct ambiguities of this kind in the thesis of physicalism, Carnap has developed the use of the reduction-sentence in his latest long paper, " Testability and Meaning ", which attempts a systematization of the principal conclusions of scientific empiricism to date. Here he presents the thesis of physicalism as the possibility of the reduction of all scientific predicates to a sufficient basis of physical predicates. The position is to be upheld thus by the use of the reduction-sentence, and, in this sense, the reduction-sentence comes to constitute the formal hub of the developed thesis of physicalism. Since the publication of this paper, however, various objections have been raised against the reduction-sentence ; and here I wish to consider one raised specifically by Prof. C. H. Langford at a meeting of the American Philosophical Association. Since the objection is connected with certain criticisms of logical empiricism made by others, it now constitutes an issue to be dealt with.

The use of the reduction-sentence concerns the introduction of a new term into the language of science on the basis of certain previously defined terms. To take Carnap's example : if we wish to introduce the predicate " soluble " into our language, supposing that we have already defined the predicate ' x dissolves in such and such ', we do not construct an explicit definition (' x is soluble if and only if . . . '), but a sentence of the form, ' If a body is placed in water, then if it dissolves, it is soluble '. In Carnap's symbolism : ' $Q_1 \supset (Q_2 \supset Q_3)$ ', where ' Q_3 ' is the predicate to be introduced and ' Q_1 ' and ' Q_2 ' are previously defined predicates. We need not consider here the distinctions in kind

of the reduction-sentence, the reduction pair or the bilateral reduction-sentence. The simplest form of reduction-sentence as above is sufficient for our discussion, and we transform it for convenience into '$(Q_1.Q_2)\supset Q_3$'. Now Prof. Langford's criticism concerns the use of the connective here, the symbol of material implication, so-called. This junction is in one equipollent form '$\backsim((Q_1.Q_2).\backsim Q_3)$' : the positive empirical content of the sentence being thus that 'where Q_1 and Q_2 are found, then Q_3 will "as a matter of fact" be found'. But the scientist, so it is said, seeks more than this mere "matter of fact". He tries to discover some empirical *causal* relation between his predicates ; and such a causal relation, the criticism goes on, is not stateable as an extensional connection. The question is thus directed at the adequacy of the extensional connective of proper implication (together, of course, with the usual junctions of disjunction and conjunction) as a formalization of the empirical language of science. The formal question here, however, in appearance so slight, is made the vehicle of a much wider and more general objection.

There are two matters easily associated with this criticism. The first is illustrated in the reduction by the Mills, and associationist psychology generally, of causal relations to relations of co-existence and succession. Mill's basic position (and, of course, Hume's before him) on this point is not difficult to understand, the bias of his analysis being here genuinely empirical. Given the various objects in nature functioning in relation to one another according to some understandable structure, Mill quite steadfastly held to the simple fact that all of this structure we ever succeed in identifying consists of the observable uniform co-existences and successions of phenomena. But in the interests of empiricism Mill here nevertheless succeeded in producing a really Metaphysical thesis : causal relations, in effect, were to be *nothing but* mere co-existence and succession, efficient connection between different events being denied. And at this point we cannot venture either to defend or attack, since the position appears to pass beyond the possibility of empirical decision. Clearly, the ideal here would be to maintain just that aspect of Mill's position which is essential to empirical purposes, without venturing into statement in the material mode about nature being with or without causes, etc.

The second matter connected with this criticism of an extensional empirical language concerns methodology rather. There is the familiar warning against the statistical fallacy of accidental correlations. We may have thus such a high positive correlation as between the membership in the International Association of Machinists and the death rate in Hyderabad, India. But, this criticism insists, we must have some " reason ", some " relation of identity ", to suppose that there is a " real connection " between the two correlated phenomena. Now the reduction-sentence with its extensional connective—and generally an extensional empirical language—would seem to leave the way open for such accidental correlations. And against this it is urged that science quite evidently demands more than mere correlation, and does succeed in obtaining " plausible "[1] and " rational "[2] connections.

Now it can be shown that the particular objection here to an extensional empirical language commits a confusion between questions of logic and of fact, and that, to the contrary, the extensional junction in the reduction-sentence, and an extensional language in general, are quite adequate to express all that empirical science requires. For this Carnap's general treatment of intensional functions may be used, but we have to supplement it by considerations of fact and procedure, not confining ourselves purely to syntax ; for the question of intensional functions is put here in a new context where we have to consider the translation of sentences with empirical content and of the form " If . . . , then . . . " into a particular formal symbolism.

2. Consider the reduction-sentence '$(Q_1.Q_2) \supset Q_3$', which we may abbreviate to '$P \supset Q_3$', this being equipollent to '$\backsim(P.\backsim Q_3)$'. Now the criticism is quite just which insists that the symbol of proper implication is not a translation of the English " If . . . , then . . . " form, in which empirical causal laws are perhaps generally stated. It is even true, moreover, that Carnap himself gives the impression of taking it as such a translation[3] ; true, too, that his paper does not provide in

[1] Meyerson, *Identité et Realité*.

[2] M. R. Cohen, *Reason and Nature*. Mr. Langford's criticism is thus seen to express the formal kernel of meaning to this usual objection to the positivistic account of science, that it is concerned only with the ' fact ', and not the ' reasoned fact '.

[3] Cf., e.g., his refutation of older positivism, " Testability and Meaning ", pp. 463–8.

exposition the way by which this difference is to be overcome. But it may be pointed out, first, that the reduction-sentence (with the exception of the bilateral reduction-pair) has no factual content, that 'Q_3' is introduced into the language by the sentence '$\sim(P.\sim Q_3)$'. There is, of course, the possibility that Q_3 may be a property already known by acquaintance and that the reduction-sentence contains some factual content in this possible agreement or disagreement with normal usage. The claim for intensionality can be carried over to the reduction-sentence without this qualification : a predicate, it is insisted, is to be introduced only through other *relevant* predicates. This insistence seems to carry over the traditional maxim (not unsound empirically) of 'definition through causes'. Thus the predicates in terms of which 'Q_3' is introduced should have some intensional relation with it, some connection of "meaning", "content", or "entailment". Even though 'Q_3' is to have meaning only through this introduction !

Suppose now we are to confirm a sentence '$P \supset Q$' ; we are told that what we have to show is an *impossibility* for '$(P.\sim Q)$'. But the requirement shows itself to be otiose, since we attempt to do what it asks merely in trying to establish the truth of '$P \supset Q$'. If '$P \supset Q$' is true, then it is "impossible" that '$(P.\sim Q)$' be true. To the degree that the truth of '$P \supset Q$' is shown, the 'impossibility' of '$(P.\sim Q)$' is shown. It is a simple proof for Semantics that from 'True ('$\sim(P.\sim Q)$')' we can derive the full negation of '$(P.\sim Q)$', and, therefore, on the basis of the given class of premises, '$(P.\sim Q)$' is contradictory. So in the quasi-syntactical form, the state of affairs designated by '$(P.\sim Q)$' is impossible. This analysis might have been known to the proponents of intensionality through classical sources, since the application of this semantical theorem goes back as far as Aristotle's use of the *per impossibile*.

In the case where we are able to demonstrate any sentence like '$P \supset Q$', and it is thereby valid, then any full sentence of 'Q' is a consequence of a full sentence of 'P'. (Carnap's theorem of analytic implicates, *Logical Syntax*, p. 43, can be developed for "valid" in a P-language). Undoubtedly this is the relation which the proponents of intensionality really want. Consider now what is the approximation to the above state of affairs which obtains in the domain of empirical fact and procedure. Here

it is the general case that universal sentences are not confirmed in isolation ; other sentences in the body of knowledge are confirmed along with the given sentence. In practice this usually takes the form that other sentences are used to confirm '$P \supset Q$' in such a way as to serve as premises in a proof of '$P \supset Q$', along too with the parallel confirmation of instances of '$P.Q$'. The scientist in confirming '$P \supset Q$' seeks to discover certain predicates, 'P_1', 'P_2', 'P_3' . . . 'P_n', which form a series of implication-sentences whose first member is 'P_1' and last member 'Q', as '$P_1 \supset P_2$' . . . '$P_n \supset Q$' ; and '$P \supset Q$' is then a consequence.[4] In the material idiom : we seek to discover certain factual conditions which are causally related to what is designated by 'P', or may even be said to cause the fact designated by 'Q'. To follow out the example concerning solubility : the chemist may confirm the sentence, 'All gases form solutions with each other in all proportions' (where, of course, there is no chemical combination), by confirming certain thermodynamic properties of available energy, or certain molecular properties, etc., which thus show "the cause of the fact" ; i.e. the sentences designating such facts allow us to derive the given sentence in question. The empirical procedure is thus to discover certain other facts which can be related with the given fact, such that we are able to construct a proof with the sentence designating that fact as a concluding sentence. Moreover, at any stage of development of empirical science, the observation-sentences which may be taken as protocols cannot, by empirical requirement, contain intensional relations : the observations expressed can only be of co-existences and successions.

Now when, in the light of the above analysis, we return to the insistence that science seeks to establish intensional connection between its predicates, that the "content" or "meaning" of P must imply Q, etc., we can separate the assertion into two parts, and evaluate it accordingly. (1) We may talk directly of the empirical facts themselves, in which case we try to find other observable properties which consistently appear together with P and Q. Here, of course, we do not find any "content" of P and Q as an empirical fact, but only other concrete properties connected with P and Q : a "causal structure" is not discerned

[4] For the general proof see Hilbert and Ackermann, *Grundzüge der Theoretischen Logik*, p. 24 ; pp. 37–42.

as an additional special fact (parallel with the observable properties connected with P and Q) within the empirical situation. (2) From the formal point of view, we find that certain other predicates and sentences (with a relatively high degree of confirmation) can be laid down to serve as proof for the given sentence. Only when these two distinct aspects of the problem are not distinguished, do we begin to speak of the content[5] of 'P' implying or causing 'Q'. The notion of "content" appears here to be reified; and this reification of "content" follows from speaking as if this "content" were discerned as an added fact within the empirical situation. That is, from the confusion of issues of fact and of language. The logical entity comes then to be considered almost as producing the empirical fact.

Far from showing any loss of expressiveness following from the use of an extensional empirical language, the various claims for intensional sentential relations in empirical science are themselves not free from a serious ambiguity.

University of Chicago

[5] "Content" is spoken of non-formally, but what is meant, I suspect, is, if translated into formal terms, L-content. Where in this paper we have spoken of the content of a predicate we meant simply the content of the class of all full expressions of this predicate.

REPLY TO MR. MACIVER[1]

By MARGARET MACDONALD

MR. MACIVER maintains that whether two token-words are instances of the same type or are 'the same word' depends not on any formal characteristics, such as physical similarity between them, but on whether they have the same *semantic value*, i.e. make the same contribution to the meaning of the sentences in which they occur (p. 63). He admits that their approximation in appearance, pronunciation, etc., to an accepted norm for that word in a language will determine whether two tokens are of the same *formal type*. I think he would also agree that spoken tokens must be associated by convention with written tokens of the same type in order to be instances of 'the same word'. But he goes on to maintain that, in the strictly philosophical use of the word 'type', neither spoken nor written tokens are instances of the same word unless they are also of the same *semantic type*, i.e. have the same meaning (p. 61). But if they do have the same meaning they are instances of the same word even though their formal types are utterly different while similar tokens which have different meanings are instances of different words. All synonyms, therefore, (e.g. *quickly* and *rapidly*) are instances of the same type-word. For stylistic reasons we may prefer to use one of their tokens rather than another, but, since all have the same meaning, all are, strictly speaking, *the same word*. Our choice is determined solely by their different emotional effects. On the other hand, homonyms which have similar tokens but different meanings are different words, in this sense.

Now Mr. MacIver may define a strictly philosophical use of a term in whatever way he pleases. If he likes to say that when he is being a philosopher he will always call *quickly* and *rapidly* the same word or the same type, there is no reason why he should not do so. Further, there is no objection to his re-defining a philosophical term already in use if he avoids confusion by telling us that this is what he is doing. But I was concerned with the type-token, sinsign-legisign distinction, introduced by Peirce and adopted by Ramsey and others and used by them to give some account of what we ordinarily mean when we use the word

[1] *Token, Type and Meaning*, A. M. MacIver. *Analysis*, April 1937.

'word' or say that two sounds or two marks are instances of 'the same word'. The use of the technical terms 'type' and 'token' makes it possible to express the same distinction for other things than words but it does not introduce an entirely new sense of the word 'word'.

It is true that grammarians and makers of dictionaries who have not made this distinction may mislead even as to ordinary usage. The Oxford Dictionary e.g. defines a homonym as 'The same word used to denote different things; words having the same sound but different meanings'. It is clear that the first use of word = type-word and the second, token words. For only token words have *sounds* but only type words have meaning since it is only in the type sense that a word can be used more than once. This sort of dictionary definition may have misled Mr. MacIver. But, surely, no ordinary person would ever think of saying that *quickly* and *rapidly* are 'the same word' or that *theism* is a different word from *theism* because it is used in two different senses. If Mr. MacIver wishes to use the type-token distinction in this way he is not using it so as to give an analysis of what we ordinarily mean by 'word' or in the way in which it has been used by philosophers, but in an arbitrary sense of his own. Does Mr. MacIver really need to know theology and medicine in order to know how many times the word *theism* occurs on this page? He asks me how, in two poems of Edward Lear, I should determine whether the same word 'runcible' occurred twice or whether two different words 'runcible' occurred. I still maintain that in the ordinary sense of 'word' tokens similar to 'runcible' are instances of the same word and I should determine the number of occurrences by counting them. There is one word 'runcible' which occurs in all the copies of Edward Lear's poems and there is one word 'theism' though both may have different meanings or no meaning may have been given to either. To deny this is to make nonsense of the type-token distinction in the use of 'word'. Of course, to say something about 'the type-words *theism* and *runcible*' is to say something about marks like *theism* and *runcible* and their relation to spoken tokens. It is not to assert the existence of an object apart from all tokens. But whether tokens are of the same word or not is determined by comparing tokens and not by reference to meanings.

According to the type-token distinction the words ' the ' and ' the ' are instances of the word *the*, in the sense in which there is only one word *the* and one definite article in English. Similarly, ' cat ' and ' dog ' are instances of the type-words *cat* and *dog* respectively. But, on Mr. MacIver's view, *what* are the different type-words of which ' theism ' and ' theism ' are instances and what is the one type-word of which ' quickly ' and ' rapidly ' are instances? He could reply only by giving an entirely new set of words which express their meanings. This seems to me wrong for another reason, but here it seems plainly wrong as an answer to a question which can be answered quite differently for words which do not have homonyms and synonyms. It is sensible to say ' The word " cat " is used to mean ' animal having such and such characteristics '. It is absurd to say ' cat ' and ' cat ' are tokens of the type ' animal having certain characteristics ' but it is correct to say ' cat ' and ' cat ' are tokens of the type-word *cat*.

In distinguishing a ' formal ' from a ' semantic ' type I think Mr. MacIver is confusing the sense in which two sounds are instances of ' the same sound ' and two shapes are instances of ' the same shape ' from the sense in which those same sounds and shapes are also instances of certain words. They are instances of these words because they are these shapes and sounds but to say that they are words is to say something different from saying that they are shapes and sounds. But it is not to say that they also have a meaning or exemplify a ' semantic type '. For the meaning of a word can be given only (a) by a verbal definition or proposition both of which are already combinations of words and so the method is circular, or, (b) by ostensive definition. But to say that ' red ', ' red ', are tokens of the colour patches by which they are defined does seem meaningless. Ostensive definitions are certainly necessary for the use of a language but they are not themselves words in *any* sense of ' word '. They are the means by which words are used. A type-word may perhaps be not too misleadingly described as a logical construction out of token words related by physical similarity and the conventional association of certain marks with certain sounds.[2] Mr. MacIver has not shown that the type-token distinction involves reference to meaning. He has merely

[2] Cf. Ramsey, *Foundations of Mathematics*, p. 274.

substituted another distinction of his own which seems to bear little relation to our ordinary use of the word ' word '.

There is, I think, a strong temptation to suppose that ' having a meaning ' is part of the definition of ' being a word ' because we tend to think in terms of the words we most often use. That is why it seemed to me that a nonsense-word might bring out the type-token distinction more clearly. A nonsense-word is still a *word* but it is a word to which no meaning has yet been given. ' Brillig is a nonsense-word ' is not a self-contradictory statement like ' This is a round square.' And the type-token distinction certainly applies to ' brillig ' as well as to ' theism '.

I do not myself wish to confine meaning to ' reference to objects ' (p. 61). I tried to show, rather, that meaning is determined by different sorts of linguistic rules and that the triadic relation of person, symbol, object may be very philosophically misleading.

Cambridge

ERRATA

In ' Value-Statements ' by Justus Buchler, *Analysis* Vol. 4 No. 4, April 1937,

p. 50, l. 15, for ' were ' read ' are ',

p. 54, l. 15, for ' not assert ' read ' not only assert ',

p. 54, l. 26, for ' $\phi x\ \psi x$ ' read ' $\phi x \supset \psi x$ '.